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ON THE CONDITIONS OF SUCCESS IN THE CONSTRUCTION AND THE COMPARISON OF STANDARDS OF LENGTH.

By PROF. WM. A. ROGERS, Cambridge, Mass.

At the first meeting of this Society, initial action was taken in regard to the adoption of a uniform standard of measurement in all microscopical work. Finally, a committee, with power to act, was appointed from the various local Societies of the country,—a committee notably strong in its composition, and promising the most valuable results. The relation of the committee to this Society is not very clearly defined, but by receiving the reports of its Secretary, Dr. Ward, it has become identified with the projected scheme, and is entitled to its share of credit in the successful completion of the work.

When I was solicited by the Boston Society to become a member of this committee, I declined, because I had already undertaken the solution of the problem of the construction of a standard yard and a standard meter, with subdivisions into aliquot parts. It has always seemed to me that a standard unit, such as that sought by this Society, should rest upon either national or internationl authority, and not upon the authority of a private individual. As I desired to continue my investigations on my own account and in accordance with certain fundamental methods of procedure which I considered essential to success, it did not seem proper that I should become a member of a committee which, possibly, might have occasion to review my own work. Moreover, there are decided advantages in independent investigations. I cannot too strongly emphasize the fact that every independent investigation of a given unit of length in which the necessary precautions are taken, must lead to precisely the same result. I offer with this communication the final units, both English and metric, which I have obtained, accompanied with the evidence upon which they rest. I have supposed that the committee would present to the Society at this meeting, for adoption, the standard unit, a centimeter, which they have secured from the Bureau of Weights and Measures at Washington, as the result of the investigations of Prof. Hilgard, now Superintendent of the United States Coast Survey. I have had the pleasure of seeing the preliminary standard. The lines ruled upon the polished surface of an alloy composed of platinum and iridium, are of the most beautiful character.

Here, then, are two determinations of the same unit, independently made. I have here a simple apparatus with which they can be compared. If they are found to be identical in length at a given temperature, this agreement will be gratifying to me, and most certainly add to your confidence in the result of the labors of your committee.

But, suppose they do not agree. Admitting the disagreement, we must, probably, refer it to errors on one side or the other, which may be assigned to one or all of the following causes, viz.:

1. An error in the adopted entire unit, which in this case is one meter, from which the subdivision into decimeters and centimeters has been made.

2. An error in the determination of the coefficients of expansion of the metals upon which the graduations have been traced, including errors in the thermometers employed to determine the temperature.

3. Errors in the determination of the aliquot parts of the entire unit sought.

I must here call your attention to the popular misconception with reference to the class of units which can be considered as standard. Units of various lengths are often spoken of as "government standards." The standards which are now in use, the yard and the meter, have been established by legislative enactment, but I have never heard of any attempt to establish the subdivision of these units by the same authority. The nearest approach to this is an act of Parliament declaring legal a certain relation between the length of the yard and the meter. In order to compare the length of the yard with the length of the meter, it is necessary to know the length of the Bronze yard at 32° F., or of the platinum meter of the Archives at 62°, an operation involving a knowledge of the law of expansion of each bar. It is not likely that either bar will pay much attention to an act of Parliament while expanding or contracting under a change of temperature amounting to 30°.

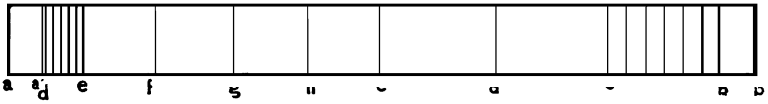
There is, therefore, no such unit as a "government foot," a "government inch," or a "government centimeter." There can only be a "government yard" or a "government meter." Any subdivisions of these units, under the authority and sanction of a department of the government, has no more value than the same work done by a private individual, unless it is more accurately done. If it is done with less perfect instruments of comparison, and by a method less exact, or with less care, it has less value. On the other hand, a certificate of comparison with the entire unit, the yard or the meter, given by an officer in charge of the original standards, must be accepted without question.

I must also call attention to another common misconception with regard to the practical necessity of extreme precision in the agreement of the entire units compared. Suppose we admit the extreme case of an error of one one-thousandth of an inch in a yard, or of twenty-five mikrons in a meter. These errors will be insensible in any microscopic measurement of a given space, and can barely be detected in an inch or in a centimeter. I shall presently show that the uncertainty still remaining with respect to the agreement of copies with the original standards, is far within these limits.

It is obvious, therefore, that the errors of importance which are the most likely to occur, are those which relate to the subdivision of the entire unit into equal parts, and those which relate to the law of expansion, under which the standards suffer a change of length under a change of temperature.

It is not difficult to obtain the subdivision into equal parts with great precision if the following conditions are fulfilled, viz.:

1. The minor unit sought must be derived directly from the standard bar upon which are traced the entire unit to be subdivided, in order that the effect of temperature may not be neglected in the investigation. Here my practice differs from that followed in the production of the platinum-iridium centimeter prepared for the committee. By the ordinary methods of observation it is impossible to obtain the coefficient of expansion with sufficient precision from so short a piece of metal. When a minor unit is an integral part of the entire yard or meter, the law of expansion is directly proportional to the number of minor units composing the entire unit of length. My investigations have so far extended to standards traced upon copper, brass, steel, glass and iron When a minor unit is desired upon either of these metals, it is my custom to transfer it to a metal of convenient shape and dimensions, and then compare it with the corresponding sub-division of the entire unit upon the same metal. After many experiments I have finally adopted the following scheme of subdivisions in a combined English and metric standard :



METRIC.

a b = 1 Meter.
a c = c b = ½ Meter.
a e = e f = f g = g h = h c = 1 Decimeter.
a d = d e = 5 Centimeters.
d e Subdivided to Centimers.

ENGLISH.

a' b' = 1 Yard.
a' c = c b' = ½ Yard.
c d' = d' e' = e' b' = 6 Inches.
e' b' Subdivided to Inches.

Diagram Illustrating Divisions of the Yard and Meter.

2. It is of the utmost importance that the surface of the standard bar which is to receive the graduations shall not only be a plane surface, but that it shall remain a plane surface throughout the investigation. The enemy of a long plane surface is flexure. If a long bar is supported at two points, the upper surface will be convex, and the lower will become concave, and the amount of curvature will depend upon the distance between the two points upon which it is supported. If the distance of the supports from the center of the bar is equal to

$$\frac{\text{The length of the bar}}{2\sqrt{3}}$$

If a plane is passed through the center of the bar longitudinally, called a neutral plane, it will remain a plane surface whatever may be the amount of the flexure at the surfaces.

The plan which I have adopted to secure the maintenance of the graduated surface of a standard bar in an invariable plane, is the following:

It is a matter of supreme importance to be able to establish a *plane of reference*, with which any other plane surface can be compared.

The bed of my first comparator is sixty inches in length. A sliding plate carrying a microscope moves upon V-shaped ways the entire length. Beneath the bed-plate there are screws to take up the vertical flexure. Now it is obvious that if this "microscope plate" can be made to move in a true plane, we shall have a plane of reference with which any other surface may be compared, and to which any other surface can be worked. The following tests of a departure from an invariable plane in the movement of the slide from one end of the ways to the other have been applied:

(*a*) If a telescope is mounted on this moving slide and pointed upon the cross-wires of a collimator or upon a well-defined distant object, the point will remain stationary during the motion of the sliding plate upon the ways. It will be seen that in this way both the horizontal curvature of the ways and the vertical flexure of the bed-plate can be detected and measured, if the telescope is furnished with a micrometer.

(*b*) Another method which I have employed with considerable success consists in the use of a shallow dish of mercury extending the entire length of the bed. If the surface of the mercury is adjusted so that it will be in the focus of the microscope at the ends of the ways, any departure from a true plane at the intermediate points will be indicated by a change of focus. With a one-fourth inch objective, it is possible to focus upon the minute globules of mercury with great sharpness.

(*c*) A third test has been found to be the most satisfactory of all. Mr. Geo. Clark, of the firm of Alvan Clark & Sons, made for me a polished surface, optically plane *when supported at its neutral points*. When this bar is placed upon the comparator, supported at its neutral points, it furnishes a plane of reference with which the plane described by the "microscope plate" can be compared.

The three tests described, combine in giving evidence that the moving slide can be made to describe a true plane when the flexure is taken up by means of the flexure screws.

I may add, also, that an excellent surface plate 42 x 1½ inches, made for me by Professor Morris, of Cornell University, remains in perfect focus under a high-power objective throughout its entire length.

In order to show that sufficient attention has not been paid to this matter in the construction of standards, I will mention a single fact which has come within my own experience. In 1880 I purchased of M. Froment, of Paris, the identical meter on which Augstrom based his measures of wave-lengths, and on which depends many of the physical measurements which have been made in France. It is an end measure, and has been compared indirectly with the legal standard of France, the "Metre des Archives," kept at the Conservatory of Arts and Meters. To the upper surface of this bar M. Froment transferred his standard line measure meter, subdivided to decimeters. I was present during the comparison of this transfer with the original, and at that time I was confident that the claim of M. Froment that the error of the subdivision was less than two mikrons, or eight one hundred-thousandths of an inch, was well founded. But when I placed this bar upon my own comparator and adjusted the end lines in focus, the lines along the middle of the bar nearly disappeared. In fact the curvature was so great that an error of one one-thousandth of an inch was introduced in the measurement of the subdivisions. I have since reconstructed this standard for my own use. In doing this I found that the middle of the bar was twelve one-thousandths of an inch higher than the surface at the ends. The original trouble must have been with the comparator of M. Froment. Probably the ways had nearly the same curvature as the surface of the bar, and on that account this serious error escaped detection.

How, then, do I prepare the surface of a bar so that its upper surface will remain in an invariable plane? I place it upon the comparator, supported at its neutral points, and then work the surface down by grinding with soft stone till every point remains in focus as the "microscope plate" traverses the ways of the comparator. If the graduations are not numerous, as in the sketch given on page 234, the labor involved is not very great, since it is necessary to grind only those parts at which the graduations occur.

It is my practice to keep standards supported at their neutral points when not in use; but it is not necessary, since the upper surface will always fall into the original place soon after the supports are applied.

3. In the transfer of the graduations on one bar to another bar, it is my practice to attach the tracing apparatus directly to the "microscope plate." It might be naturally supposed, therefore, that it is possible to make the transfer as accurate as it is possible to set the cross-wires of the microscope micrometer upon the lines of the original bar.

This would be the case if it were not for one serious source of error. While the "microscope plate" may be made to move in a true plane in the manner already described, it may not move in a straight line on account of the horizontal curvature of the ways along which the "microscope plate" moves. Hence, when a horizontal cross-line is traced upon the bar, it may be a curved line. Since the measurement of the subdivisions is made from the intersection of this curved line with the vertical line which marks the subdivisions, it will be impossible to obtain the relation of the subdivisions to the entire length of the given unit until the law of curvature is known. In my first comparator the radius of curvature is five and one-half miles, and yet the error introduced in comparing one meter with another is no less than five mikrons when one bar is two and one-half cetimeters farther from the center of the ways than the other. I have yet to find a planer which will plane long ways sufficiently accurate for the purpose of measurements of precision. I have long been convinced that it is possible to make cylindrical ways practically straight. Hence, in the Rogers-Bond Universal Comparator, built by the Pratt & Whitney Company, of Hartford, I have employed hollow cylinders of steel four inches in diameter, supplied with flexure levers, by which the vertical curvature can be neutralized. The result has justified my most sanguine expectations. This comparator is so constructed that lines forty inches in length can be traced on opposite sides of the ways as far as twelve inches from the center. Tracing a line upon a bar in one position and then removing the bar to the opposite side of the ways, and, after adjusting it parallel with the ways at the two ends, tracing another line at a distance, for example of one one-thousandth of an inch from the first line, it is found by an examination under the microscope that the two lines are nearly parallel; whereas, if the ways have a horizontal curvature, the lines would separate at the middle point by an amount equal to twice that due to the curvature. I am confident that it is impossible to depend upon V-shaped ways of the ordinary form with the expectation that they will yield good results under this severe test.

There is, however, one form of construction which will admit of great precision. If the two faces are made independently, and are afterward placed in position to receive a moving plate which touches one face at three points and the other face at two points only, this sliding plate may be made to move in a true plane. A knife for cutting thin sections is sometimes mounted in this way.

If the sliding plate rests upon the cylindrical guides by segmental bearings, friction is reduced to a minimum, and the motion of the plate becomes very steady. I have recently introduced into my machine for the original graduation of circles, a modification for the purpose of ruling some plates of large dimensions, to be used in connection with the photographic apparatus to be employed in the forthcoming transit of Venus. In this machine I have two sets of cylinders twenty inches in length. By reversal in the manner indicated above, it is found that lines ruled twelve inches in length are absolutely straight. I submit, therefore, that the evidence of actual trial which I offer is conclusive in showing that the horizontal curvature can be overcome by the use of cylindrical ways, while the vertical curvature can be overcome by flexure levers. Cylinders can be made straight and of a uniform diameter either by the process invented by Morton Poole, of Wilmington, Delaware, or by being ground by a series of rings, the cylinder being kept in a vertical position during the operation.

There is another fruitful source of error which is even more important, since it may be nearly as large in a minor unit as in the whole unit to be compared. I designate it "error of focus." In my new comparator I have the means of changing the focus of a defining line at one end of the bar, while the focus at the other end remains unchanged. The time required to make the change does not exceed five or six seconds. As the bar is not touched during this time, it is possible to study the effect of a change of focus upon the length of the bar with great ease and certainty. The results which I have obtained in this way with the new comparator form the most important contribution which it has made in the field of precise measurements. The general result may be stated as follows: The error of focus varies with the size of the line, and with the power of the objective. It has been almost the universal practice to make the lines of a standard from fifteen to thirty mikrons in width. With lines of this width, and with a low-power objective, I estimate the error of focus to be at least five times as great as with a line one mikron in width, under a magnification of five hundred diameters. The question whether high or low powers are to be preferred in precise measurements, has yet to be argued and settled by metrologists. When that time comes, I shall have something to say. Meanwhile, I do not hesitate to say that the use of the very lower powers, which has almost universally prevailed, has been the means of introducing errors which with higher powers would not have escaped attention. Even if the edges of a line appear sharp and well defined under a low power, this is not an evidence that another line, which seems to appear equally well defined, is at the same focal distance. Just here I am reminded of my experience with the carpenter who "leveled up" my first comparator. I furnished him with a delicate astronomical level. After a while, he came to me in great disgust, saying that the level which I had given him was good for nothing—that it "bobbed all about," and that he could do nothing with it. "But," said he, "I have a level at home that is a good one—one that will settle down to the same spot every time." So he insisted on going home after a level that "would come to the same spot every time." I hardly need to add that, after he had finished his work with a common carpenter's level, I was obliged to do it over again with a more delicate level.

At this point it is proper to inquire, What is the limit of accuracy attainable in the comparison of different units of length? That depends. If the observations are repeated under exactly the same conditions, the apparent error is much smaller than when they are repeated under different conditions, and by different persons. I must here enter my protest against the use of the term "probable error," in the sense which it has in some way secured for itself in the popular estimation, viz., as synonymous with the actual error. The value of the "probable error" is found by the following rule: From the mean of any number of values of a given quantity, subtract each value of this quantity; divide the sum of the squares of the residuals thus found by one less than the number of residuals, and extract the square root of the quotient; the result will be the "probable error" of a single observation; divide this result by the square root of the number of observations, and the quotient will be the probable error of the mean value.

Suppose we have the following observed settings of the microscope micrometer upon a single centimeter, viz.:

	Mikrons.	Residuals.
	1000.0	0.4
	1000.2	.2
	1000.1	.3
	1000.6	.2
	1000.5	.1
	1000.9	5
	1000.1	.4
	1000.4	.0
	1000.3	.1
	1001.0	.6
Mean...	1000.4	2.8

Then $\sqrt{\frac{2.8}{10-1}} = 0.31$ mikron = probable error of a single observation.

$\frac{0.31}{\sqrt{10}} = 0.10$ mikron = probable error of the mean value 1000.4.

It is exceedingly difficult to remove the popular impression that the value 1000.4 mikrons is correct within the limit 0.10 mikron. Owing to causes such as those enumerated above, the real error may be as great as five mikrons, or fifty times as great as the probable error of the mean.

I do not need to remind those who are familiar with the mathematics of this subject, that the term "probable error" has only a relative signification—that constant errors of every kind disappear in taking the mean from which the residuals are obtained. Hence it must always be understood that there is to be added to a computed value of the probable error an undetermined and, in most cases, an unknown constant.

In a joint paper presented by Prof. E. W. Morley, of Adelbert University, to the American Academy of Arts and Sciences, in 1878, an attempt was made to ascertain the ultimate limit of accuracy in measurements with the microscope. I ruled a series of plates containing bands, in some of which periodic errors were purposely introduced. in others of which the interlinear spaces were made as nearly equal as possible. Each observer measured the 235 spaces given as tests with the greatest care. After the reports were written out for publication, the results were for the first time compared. The following are the general results which were obtained:

(*a.*) The probable error of the measurement of a single space was found to be 19 ten-millionths of an inch; the greatest deviation from the mean value being eight-millionths of an inch.

(*b.*) The average disagreement between Professor Morley and myself was found to be 34 ten-millionths of an inch, and in all but eight cases the disagreement was less than one hundred-thousandth of an inch. The degree of agreement will be seen from the following figures :

Number of millionths = 0 1 2 3 4 5 6 7 8 9 10 11 12.
Number of cases of agreement = 16 30 39 24 15 17 13 8 2 3 4 2 2.

In this experiment the conditions were all favorable. The lines were fine and sharply cut, the entire space measured did not exceed one-tenth of an inch, there was no trouble with flexure, and the illumination was entirely within control. In the ordinary work of comparing standards, this degree of accuracy is not attainable. It is perfectly easy to measure a given unit within one fifty-thousandth of an inch. I am *never* sure that a given measure is true within one hundred-thousandth of an inch. I have no faith in the trustworthiness of figures in the millionths place, though it is quite possible that they may have some significance when they result from the mean of a large number of observations.

Having considered the conditions of success in the investigation of the subdivision of a given unit into aliquot parts, let us take up the second branch of our subject, viz., the determination of the law of expansion of the metals upon which the graduations are traced. Let us first make it plain why it is absolutely necessary that the behavior of a given bar under a varying temperature shall be known.

With unimportant exceptions, there are but two units in which measurements of length are expressed, viz., the yard and the meter.

The Yard.—Without going into a history of the British yard as first legally established, of its destruction by the fire which consumed both houses of Parliament in 1834, or of the processes by which it was restored in 1855, it is sufficient to say that, wherever the English authority extends, there is, at this time, but one legal yard, viz., the bronze bar now deposited in the "Strong Room" in the Old Palace, at London. This particular bar, made a standard by a special act of Parliament, is the ultimate authority in all measurements of length in which the unit is one yard. This bar is thirty-eight inches long, one inch wide, and one inch deep. It is made of composition known as Baily's metal, composed of sixteen parts of copper, two and one-half parts of tin, and one part of zinc. It is standard at 62° F.

When this standard was made, there were also constructed, at the same time, forty other bars, which were carefully compared with it. By order of the Board of Trade, several of these supplementary standards were distributed to different governments and societies. The bar known as "Bronze 11" came to the United States. At that time it was declared to be about one ten-thousandth of an inch longer than the present Imperial yard; but, within the past three or four years, two independent comparisons have been made between the two yards by Prof. Hilgard and by Mr. Chaney, the Warden of the Standards. Whatever may have been the original relation, it is certain that Bronze 11 is now about eighty-eight millionths of an inch *shorter* than the Imperial yard.

There is no such unit as the "American Yard." All comparisons at the Bureau of Weights and Measures are now made with Bronze 11, and thence with the Imperial yard, through the relation: Bronze 11 + .000088 inch = Imperial yard.

It is hardly necessary to say that all attempts to express the yard as a legal unit, in terms of the length of a seconds pendulum, as originally proposed, have been abandoned.

The Meter.—The meter, as originally established in 1795 by a commission appointed by the French government, was supposed to be one ten-thousandth of a quadrant of the earth's surface in length at the temperature of 32° F., or 0° C. At that time, two meters representing this length were made of pure platinum, having the dimensions 100 × 2½ × 1 centimeters. They are, therefore, known as end measures. The end-faces are supposed to be parallel, and a meter is the perpendicular distance between the two faces at 0° C. One of these meters, known as the "Metre des Archives," is the legal standard of France. The other is deposited at the "Conservatoire des Arts et Metiers," and is known as the meter of the Conservatory. The evidence is pretty conclusive that, at the time these bars were made, they were supposed to be equal in length; but the meter of the Conservatory is now found to be 3.2 mikrons longer than the meter of the Archives.

In 1870 a commission was established, under the authority of thirteen different governments, for the purpose of securing the adoption and distribution of a uniform system of weights and measures throughout the world. An extensive and well-appointed Bureau was established at Breteuil, near Paris. Among the first acts of this commission was the resolution that the Metre des Archives should be taken as the basis of the international meter. Accordingly, the French section of this commission undertook the labor of obtaining the equivalent of the meter of the Archives in a line measure traced upon a bar composed of an alloy of platinum and iridium, of the form proposed by M. Tresca. This form is generally spoken of as X-shaped, but a more exact, though more homely, designation is that of the common wood saw-horse. The piece connecting the two ends may be considered to be in a neutral plane with respect to the whole mass. Inasmuch as it was not permitted to make metal contacts with the Metre des Archives, on account of the risk of a defacement, which would interfere with the definition of the whole length, this transfer required the utmost care, patience and skill. It was, in fact, the vital part of the process of the construction of the international standard. Instead of attempting to compare all of the different line-measure standards which would be required in the operations of the Bureau with the original meter, the plan of comparing a single one with the greatest care and precision, was more wisely chosen. This work was done by M. Tresca, at the Conservatory of Arts and Meters, upon a combined comparator and tracing machine, built by the celebrated mechanician, M. Froment, of Paris. This work involved the labor of many months. The line meter thus constructed is designated a "working meter," because it is from this bar that the various prototypes of the International Bureau have been constructed. Practically, this particular "working meter" is the unit upon which the metric system is based. It will, of course, rest with the International Commission to recommend this, or any other bar as the final standard to be adopted by legislative enactment, but, since this bar is now identical in length with the meter of the Archives, the variation of any particular bar adopted can be of no importance, even in the most refined scientific investigations.

It will be seen that no attempts have been made to make the new meter conform to the definition of the meter of 1795. Like the yard, its authority rests upon arbitrary enactment, and in this respect it has the same justification as an original unit, as the yard, and no more, with the exception that the yard is the older unit. It is, probably, on this account that Great Britain has declined to enter the confederation which has established the International Bureau, and has resolved to adhere to the yard as a unit. In any argument with reference to the relative advantages of the two systems, the English and the Metric, this fact must be taken into account. Briefly stated, the advantages claimed for the metric system consist wholly in its decimal subdivisions,—in the unity of the system of weights within the system of measures,—and not in any supposed superiority of the entire unit upon which it rests.

Since the yard is traced upon a bronze bar at 62°, and the meter upon a platinum bar at 32°, it is necessary, in order to compare the two units, either that the amount of the expansion of the platinum bar for 30°, or of the contraction of the bronze bar for 30°, shall be known. The subject of the relation between the length of the yard and the meter is at present involved in a good deal of uncertainty, since the original standards have never been directly compared. I hope to be able, in a paper shortly to be published, to add something to our present knowledge of this relation. According to the present evidence, the accepted values are all too large. The true relation cannot differ much from 39.37015 inches.

Upon the threshold of any attempt to make a practical use of the standard yard and the standard meter, we are met with the fact that in the great mechanical industries of the world brass plays comparatively an unimportant part, while platium is almost wholly unknown. Steel and iron are the metals with which we must deal, and whose behavior under varying temperatures we must know. Hence, it is necessary to find the equivalent of the original standards when transferred to bars of iron, steel and glass. This is the work which has occupied my attention for the past three years. The outcome of this labor is the combined yard and meter which I have the pleasure of exhibiting at this meeting.

Some idea of the necessity of a thorough investigation of this kind may be gained from the statement that, while two brass yards can be compared within the limits of ten or fifteen-millionths of an inch, if the temperature is constant, the expansion of the bar for each degree of temperature is four hundred-millionths of an inch. In no part of this investigation is there a greater liability to error than in the treatment of the question of temperature.

First of all, there may be an error in the thermometers employed. The thermometers may read differently when placed horizontally and in a vertical position. It is generally assumed that when a thermometer is placed upon a bar horizontally, it indicates the temperature of the bar. But this is by no means necessarily true. The thermometer simply indicates its own temperature. The heat of the body of the observer may effect the reading of the thermometer without exerting any perceptible influence upon the bar till long afterward. Hence the common practice of reading the thermometer at the beginning and the end of a comparison, and then using the mean value is radically wrong and cannot be defended. Another common error is introduced by neglecting to take into account the fact that the temperature of the upper layer of air surrounding a bar is almost always perceptibly different from the layer at the lower surface.

The most practical remedy for these errors consists in applying the thermometers in the same way and under the same conditions as they were employed in the comparison with the original standards. Hence, I always place them horizontally upon the upper surface of any bar to be compared with my standard. My standard thermometer was standardized at several points by Dr. Waldo, of Yale College. I have also carefully compared it with a thermometer belonging to the Observatory, which was compared by Mr. S. W. Holman with the air thermometer of Professor Rowland, of Johns Hopkins University. This comparison verified every point determined by Dr. Waldo, to the single tenth of a degree given.

The authorities generally give an elaborate system of precautions necessary to be taken in the management of the question of temperature as affecting the measured expansion and contraction of a given bar whose co-efficient is to be determined. I find that one simple precaution covers the whole ground. As my practice will, without doubt, be severely and widely criticised, I desire to state it clearly and defend it vigorously in advance. It is the common practice to arrange a system of protecting screens, on the supposition that the presence of the observer near the bars to be compared will interfere with the accurate comparison, on account of the effect of the animal heat of the body in raising their temperature. The agent of Sir Joseph Whitworth, at the last Paris exposition was accustomed to say that if he should touch the standard steel bar which he exhibited with his finger nail for a single instant of time, the effect upon the length of the bar could be detected with his millionth machine with the most unerring certainty. I think I should find it difficult to construct a sentence more misleading than this, and I may add expressive of a greater amount of nonsense. In the first place, I know from personal inspection that his millionth machine will not and cannot be made to measure a given length with certainty within one fifty-thousandth of an inch, to say nothing of one-millionth of an inch. It might just as well be called a trillionth machine, for all that would be required would be to add another worm-gear to the index which measures the difference between two given lengths. It is a millionth machine only in the sense of indicating this degree of accuracy by a mechanical contrivance. You will be surprised to learn that it consists simply of a screw terminating in a face perpendicular to the axis of the screw, and set in a line with a cylinder of equal diameter having a parallel face. The millionth of an inch is got by placing, for example, a foot to be measured between these two faces, determining the true contact by the sense of feeling, and reading the index of the screw either directly or under the magnification indicated by the index of a worm-gear. I can only say of this method of comparison that I have not been able to employ it with success. Pardon this digression.

The heat of the body of the observer does *not* affect the length of a bar appreciably within the time required to make an accurate comparison of the length of two bars. While the heat of the body will act very quickly on the thermometer, it will act very slowly upon the bars themselves, the rate of the propagation of the heat waves depending upon the mass and the shape of the bar. If I remove a brass bar 38 × 1 × 1 inches from a room which has a temperature of 32°, to another room which has a temperature of 62°, the time required for the bar to reach its normal length at 62° is about 5 hours, while the copper bar of the Tresca form will reach its normal length, under the same conditions, in from twenty-five to thirty-five minutes. The odly apparent exception which I have noted to this slow movement of temperature waves as a function of the mass, is in the case of a nickel-plated steel bar having the graduations on the nickel surface. With this bar in the comparing-room at 90°, the effect of introducing through the window a wave of air at about 25°, for a period of thirty seconds only, was to increase the length over one one-thousandth of an inch.

One of the first things to be done, therefore, in an investigation of this kind, is to determine the rate of the propagation of temperature waves for all the bars under examination; and, secondly, to determine the length of time required before a change of two or three degrees can effect the relative lengths of the bars. I feel entirely safe in saying that even with my Tresca bar, which weighs only about two and one-half pounds, a change of the temperature in the room amounting to 1° or 2° will be imperceptible within six minutes. In the case of a brass bar 38 × 1 × 1 inches, the limit may be safely placed at fifteen minutes.

Now I have taken advantage of this fact to do away with many things which interfere with exact work. Through the liberality of the president of Harvard College, a comparing-room has been fitted up in the basement of the Havard Hall, which is admirably adapted for my work. I need not describe it further than to say that with repeated daily changes of from 10° to 30° in the temperature of the outside air, the total change within the room for three weeks was only 1.7°. Upon my new comparator, which is mounted in this room, I am able to compare two standards in about one minute and forty-five seconds. I generally place three standards in position at one time, on each of which are traced both the yard and the meter. The time required to make all the adjustments required, without touching either bar, and of making the six comparisons desired, is about fourteen minutes. I then leave the room and do not enter it again for about four hours, when the comparison can safely be repeated. This is the precaution which is vital to the whole system which I have adopted. The heat developed during the first visit to the comparing-room, and whose effect was insensible during that time, in the course of four hours has had time to expend its force and allow the bar to resume its normal condition. You will observe that everything depends on maintaining a constant temperature in the interval between the two sets of observations.

In a discussion of this kind it is always better to leave the dry details of the observation for the printed publication. Hence, I have thought it wiser to discuss the conditions of success rather than to burden you with their details. I shall therefore give only the outlines of the several steps by which I have arrived at the results which I have obtained. In January, 1880, I visited London and Paris in the interest of the Rumford Committee of the American Academy of Arts and Sciences. I took with me for comparison with the Imperial Yard a steel yard, and for comparison with the meter at Paris, a meter traced upon a bar of pure silver, with inlaid laminæ of both gold and platinum.

Through the kindness of Mr. Chaney, the Warden of the National Standards, I obtained from him a comparison of the steel yard with the Imperial Yard, at a temperature ranging from 50° to 68°. At Paris I found Professor Tresca engaged in the operation of constructing prototypes of the working line meter which I have already described. I was, therefore, at that time, unable to obtain a standardization of the meter on silver, but M. Tresca did me a far better service by presenting to me a meter upon platinum surfaces inserted in a bar of pure copper, of the same form as the platinum prototypes. This bar was placed upon the comparator with the working line meter of the conservatory, on the 4th of February. At 2 o'clock on the morning of February 6th the transfer was made by M. Gustav Tresca.

It was immediately compared, both by M. Tresca, and, at the request of Professor Tresca, by myself also. Comparisons were continued during the next day and night by M. Tresca. The certificate accompanying this bar states that it is 1.0 mikrons longer than the bar with which it was compared at 56°.67 F. At this temperature, therefore, it is 119.8. mikrons too long, adopting for platinum the coefficient .0000086086 mikrons.

Upon my return home I began a series of observations for the determination of the coefficients of expansion of the steel and the copper bar. I soon found that I had made a great mistake in taking with me for comparison a yard upon steel instead of upon brass. Worse than this, I had traced the graduations upon a nickel surface. About this time I undertook to establish the units required by the Pratt & Whitney Co., of Hartford, Conn., in the manufacture of their standard gauges. Recognizing the mistake I had made, I determined to commence *de novo* with the yard. Repeating as nearly as possible the conditions under which the comparisons had been made at London, I transferred a yard from the nickel-plated steel bar to a bar of the same composition, shape and size as the Imperial Yard. I sent this bar to the Bureau of Weights and Measures at Washington, with a request for comparison with Bronze 11. It was returned by Professor Hilgard, with the statement that it was one forty-thousandth of an inch shorter than Bronze 11 at 56°, or, practically, at 62°. Here, then, I had a starting point from which to proceed, nearly independent of temperature. My formal report to Messrs. Pratt & Whitney, soon to be published, will give the details of the works by which I have passed from a bronze bar to bars of steel, both tempered and untempered, and thence to the steel bar which I exhibit at this meeting. I may add that I shall have shortly the means of an independent verification of this yard with the original standard. While in London Mr. Chaney did me the great service of presenting to me, under the authority of the Board of Trade, a glass bar made by Chance & Sons ten years previously, having a definite and a known composition. I also brought home a duplicate bar, in order to graduate it for M. Chaney. This bar will soon be returned. With the known relation between the yards traced upon the two glass bars, and the comparison which will hereafter be made with the Imperial Yard, a new verification will be obtained.

With regard to the meter, I may say that since 1880 I have compared it with two meters which have been compared with the meter of the Archives; first, with the Froment end-measure meter which I purchased in Paris; and second, through the kindness of Prof. Hilgard, with a brass meter belonging to the Coast Survey, known as "No. 49." Adopting my meter as the standard, I find "No. 49" to be seventeen and five-tenths mikrons shorter than the meter of the Archives, while Doctor Foerster, of Berlin, gives the two values five and two-tenths mikrons shorter, from a direct comparison, and twenty-one and four-tenths mikrons shorter from an indirect comparison with the meter of the Archives. I am confident that the second value given by Dr. Foerster will be found to be much nearer the truth than the first one.

A few months since, I traced line meters at 32° upon two steel bars and one zinc bar, for Mr. E. S. Wheeler, of the United States Lake Survey. These meters were most carefully compared with the Tresca meter. Since they will ultimately be compared with a meter which bears a definite and known relation to the standard of the International Commission, I shall be able to obtain another verification of the Tresca meter through the relations which I have derived. It may be assumed, for the present, therefore, that the line meter in my possession is, within narrow limits, a true copy of the meter of the Archives.

The length of this communication, already too great, will prevent a full discussion of the three standard bars which I have constructed for my own use. I will, however, give the relations which I have obtained between the line measures laid off upon these bars and the original standards of the temperature of 62° F.

Glass Bar.

Yard + .00005 inch = Imperial Yard.
Meter + 2.0 mikrons = Meter of the Archives.

Brass Bar.

Yard + 00012 inch = Imperial Yard.
Meter — 1.9 mikrons = Meter of the Archives.

Steel Bar.

Yard + .00009 inch = Imperial Yard.
Meter + 3.0 mikrons = Meter of the Archives.